nelids in 1884, gives us some results of his personal experience, which I believe have never been placed before the English reader. He says that he had the good fortune once at least to observe an interesting case of phosphoresence in connection with the brandling. It was one warm July night in the year 1881, when he was exploring a dung-heap. Naturalists do not usually work with kid gloves and diamond rings. Presently a spot of soft, bluish-white light appeared, which, however, was changeful and unsteady. Now it would disappear, then return anew and shine forth over a larger space, though never with a brilliant hue. He thereupon removed a portion of the manure from the spot where he had observed the luminosity, and found that the light appeared brighter, and shone for a longer time without disappearing, or before it migrated to another spot. By means of a lantern Vejdovsky was able to secure a large number of specimens of the brandling from the dung-heap, which he placed in a vessel for the purpose of subjecting them to careful observation. To his great surprise he found that his finger soon glowed in the darkness with the phosphorescence, which extended generally over the hand where it came into contact with the worms. It was therefore apparent that the luminosity was the product of a fluid secreted by the cutaneous glands, which had attached itself to the hand of the investigator, and now manifested itself in this curious way.

We have an interesting observation on the same subject by Prof. von Stein, which was published at Leipzig in 1883. One evening in the middle of September the Professor was spending some time with a circle of friends at a parsonage not far from Potsdam, when the conversation turned upon phosphorescence and the phenomena of light. Hereupon one of the younger members of the family—who are usually the keenest and most shrewd observers of Nature, and the best friends of the naturalist —observed that there were fountains in the adjoining gardens, the water from which was frequently observed to be full of light-bearing creatures when it was violently agitated. He regarded the affair at first simply as a hoax, or an attempt to make a fool of him—as people are ever ready to do with a hobby-rider —but ascertained eventually that the luminosity was due to the presence of a species of worm which possessed the property of shining when disturbed. As with Vejdovsky, so with Prof. von Stein, the finger which had come into contact with the worm continued to glow for some time after. What species of worm was under observation is not recorded.

It now becomes a question, What end could be served thereby? The philosopher no sooner learns a new fact than he begins to pry into the secret which lies beneath, and stands to it as cause to effect. We have analogy to guide us. The water worms may be compared with the marine animals which produce phosphorescence, while the brandling may be studied in the light of the glow-worm. It may be objected that as worms have no eyes there can be no advantage in their luminosity. But such an argument would be based on the erroneous assumption that a creature without eyes is incapable of receiving impressions from light. That worms are influenced by light is proved both by their habit of avoiding light, and by the experiments which have been carried out by various students. Darwin remarks that as worms are destitute of eyes he at first thought they were quite insensible to light. He found, however, that "light affects worms by its intensity and by its duration." Hoffmeister states that with the exception of a few individuals worms are extremely sensitive to light, and from my own observations I have been able to demonstrate that there are marked differences in the susceptibility of the different species—some being very much more susceptible than others.

Now it follows that if a number of species of worms lived together in one place, as they usually do in a manure heap, it would be a great advantage for a given species to possess a distinguishing feature, such as that of luminosity, to enable two individuals to discover each other's whereabouts, just as the male glow-worm detects the female by the light emitted from her upturned abdomen. We have, moreover, the fact that certain species of earthworm are characterised by a peculiar odour, which must be of great service in preventing promiscuous copulation and hybridity. Though earthworms are destitute of nasal organs they can detect odours, and though sightless they are affected by light.

Viewed in this light a new field of research is opened up which hitherto has been totally unworked, but which may be hoped to yield remarkable results if diligently, patiently, and intelligently tilled.

It would be an easy thing for any one living in the country, with access to an old manure heap, where the brandling (*Allolobophora fœtida*, Sav.) usually abounds, to ascertain whether such luminosity is of common occurrence, and it would be exceptionally valuable to record the period of the year, the state of the atmosphere, the age of the moon, and other data which would enable the specialist to arrive at a satisfactory conclusion. I shall be glad to receive communications, addressed "The Grove, Idle, Bradford," from observers who may find pleasure in such pursuits. HILDERIC FRIEND.

Quaternions and the Algebra of Vectors.

IN a recent number of this Journal (p. 151) Mr. McAulay puts certain questions to Mr. Heaviside and to me, relating to a subject of such importance as to justify an answer somewhat at length. I cannot of course speak for Mr. Heaviside, although I suppose that his views are not very different from mine on the most essential points, but even if he shall have already replied before this letter can appear, I shall be glad to add whatever of force may belong to independent testimony.

Mr. McAulay asks: "What is the *first* duty of the physical vector analyst *quâ* physical vector analyst?" The answer is not doubtful. It is to present the subject in such a form as to be most easily acquired, and most useful when acquired.

In regard to the slow progress of such methods toward recognition and use by physicists and others, which Mr. McAulay deplores, it does not seem possible to impute it to any want of uniformity of notation. I doubt whether there is any modern branch of mathematics which has been presented for so long a time with a greater uniformity of notation than quaternions.

What, then, is the cause of the fact which Mr. McAulay and all of us deplore? It is not far to seek. We need only a glance at the volumes in which Hamilton set forth his method. No wonder that physicists and others failed to perceive the possibilities of simplicity, perspicuity, and brevity which were contained in a system presented to them in ponderous volumes of 800 pages. Perhaps Hamilton may have intended these volumes as a sort of *thesaurus*, and we should look to his shorter papers for a compact account of his method. But if we turn to his earlier papers on Quaternions in the *Philosophical Magazine*, in which principally he introduced the subject to the notice of his contemporaries, we find them entitled "On Quaternions; or on a New System of Imaginaries in Algebra," and in them we find a great deal about imaginaries, and very little of a vector analysis. To show how slowly the system of vector analysis developed itself in the quaternionic *nidus*, we need only say that the symbols S, V, and ∇ do not appear until two or three years after the discovery of quaternions. In short, it seems to have been only a secondary object with Hamilton to express the geometrical relations of vectors,—secondary in time, and also secondary in this, that it was never allowed to give shape to his work.

But this relates to the past. In regard to the present *status*, I beg leave to quote what Mr. McAulay has said on another occasion (see *Phil. Mag.* June, 1892):—"Quaternions differ in an important respect from other branches of mathematics that are studied by mathematicians after they have in the course of years of hard labour laid the foundation of all their future work. In nearly all cases these branches are very properly so called. They each grow out of a definite spot of the main tree of mathematics, and derive their sustenance from the sap of the trunk as a whole. But not so with quaternions. To let these grow in the brain of a mathematician, he must start from the seed as with the rest of his mathematics regarded as a whole. He cannot graft them on his already flourishing tree, for they will die there. They are independent plants that require separate sowing and the consequent careful tending."

Can we wonder that mathematicians, physicists, astronomers, and geometers feel some doubt as to the value or necessity of something so separate from all other branches of learning? Can that be a natural treatment of the subject which has no relations to any other method, and, as one might suppose from reading some treatises, has only occurred to a single man? Or, at best, is it not discouraging to be told that in order to use the quaternionic method, one must give up the progress which he has already made in the pursuit of his favourite science, and go back to the beginning and start anew on a parallel course?

I believe, however, that if what I have quoted is true of vector methods, it is because there is something fundamentally wrong

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in the presentation of the subject. Of course, in some sense and to some extent it is and must be true. Whatever is special, accidental, and individual, will die, as it should; but that which is universal and essential should remain as an organic part of the whole intellectual acquisition. If that which is essential dies with the accidental, it must be because the accidental has been given the prominence which belongs to the essential. For myself, I should preach no such doctrine to those whom I wish to convert to the true faith.

In Italy, they say, all roads lead to Rome. In mechanics, kinematics, astronomy, physics, all study leads to the consideration of certain relations and operations. These are the capital notions; these should have the leading parts in any analysis suited to the subject.

If I wished to attract the student of any of these sciences to an algebra for vectors, I should tell him that the fundamental notions of this algebra were exactly those with which he was daily conversant. I should tell him that a vector algebra is so far from being any one man's production that half a century ago several were already working toward an algebra which should be primarily geometrical and not arithmetical, and that there is a remarkable similarity in the results to which these efforts led (see Proc. A.A.A.S. for 1886, pp. 37, ff.). I should call his attention to the fact that Lagrange and Gauss used the notation $(\alpha\beta\gamma)$ to denote precisely the same as Hamilton by his $S(\alpha\beta\gamma)$, except that Lagrange limited the expression to unit vectors, and Gauss to vectors of which the length is the secant of the latitude, and I should show him that we have only to give up these limitations, and the expression (in connection with the notion of geometrical addition) is endowed with an immense wealth of transformations. I should call his attention to the fact that the notation $[r_1 r_2]$, universal in the theory of orbits, is identical with Hamilton's $V(\rho_1\rho_2)$, except that Hamilton takes the area as a vector, *i.e.* includes the notion of the direction of the normal to the plane of the triangle, and that with this simple modification (and with the notion of geometrical addition of surfaces as well as of lines) this expression becomes closely connected with the first-mentioned, and is not only endowed with a similar capability for transformation, but enriches the first with new capabilities. In fact, I should tell him that the notions which we use in vector analysis are those which he who reads between the lines will meet on every page of the great masters of analysis, or of those who have probed deepest the secrets of nature, the only difference being that the vector analyst, having regard to the weakness of the human intellect, does as the early painters who wrote beneath their pictures "This is a tree," "This is a horse."

I cannot attach quite so much importance as Mr. McAulay to uniformity of notation. That very uniformity, if it existed among those who use a vector analysis, would rather obscure than reveal their connection with the general course of modern thought in mathematics and physics. There are two ways in which we may measure the progress of any reform. The one consists in counting those who have adopted the *shibboleth* of the reformers; the other measure is the degree in which the community is imbued with the essential principles of the reform. I should apply the broader measure to the present case, and do not find it quite so bad as Mr. McAulay does.

Yet the question of notations, although not the vital question, is certainly important, and I assure Mr. McAulay that reluctance to make unnecessary innovations in notation has been a very powerful motive in restraining me from publication. Indeed my pamphlet on "Vector Analysis," which has excited the animadversion of quaternionists, was never formally published, although rather widely distributed, so long as I had copies to distribute, among those who I thought might be interested in the subject. I may say, however, since I am called upon to defend my position, that I have found the notations of that pamphlet more flexible than those generally used. Mr. McAulay, at least, will understand what I mean by this, if I say that some of the relations which he has thought of sufficient importance to express by means of special devices (see Proc. R. S. E., for 1890–91), may be expressed at least as briefly in the notations which I have used, and without special devices. But I should not have been satisfied for the purposes of my pamphlet with any notation which should suggest even to the careless reader any connection with the notion of the quaternion. For I confess that one of my objects was to show that a system of vector analysis does not require any support from the notion of the quaternion, or, I may add, of the imaginary in algebra.

I should hardly dare to express myself with so much freedom, if I could not shelter myself behind an authority which will not be questioned.

I do not see that I have done anything very different from what the eminent mathematician upon whom Hamilton's mantle has fallen has been doing, it would seem, unconsciously. Contrast the system of quaternions, which he has described in his sketch of Hamilton's life and work in the *North British Review* for September, 1866, with the system which he urges upon the attention of physicists in the *Philosophical Magazine* in 1890. In 1866 we have a great deal about imaginaries, and nearly as much about the quaternion. In 1890 we have nothing about imaginaries, and little about the quaternion. Prof. Tait has spoken of the calculus of quaternions as throwing off in the course of years its early Cartesian trammels. I wonder that he does not see how well the progress in which he has led may be described as throwing off the yoke of the quaternion. A characteristic example is seen in the use of the symbol ∇. Hamilton applies this to a vector to form a quaternion, Tait to form a linear vector function. But while breathing a new life into the formulæ of quaternions, Prof. Tait stands stoutly by the letter.

Now I appreciate and admire the generous loyalty toward one whom he regards as his master, which has always led Prof. Tait to minimise the originality of his own work in regard to quaternions, and write as if everything was contained in the ideas which flashed into the mind of Hamilton at the classic Brougham Bridge. But not to speak of other claims of historical justice, we owe duties to our scholars as well as to our teachers, and the world is too large, and the current of modern thought is too broad, to be confined by the *ipse dixit* even of a Hamilton.

J. Willard Gibbs.

Glacial Drift of the Irish Channel.

It seems of interest to record that the eurite or microgranite containing blue amphibole (Riebeckite), the rock noticed by Mr. P. F. Kendall in the drifts of the Isle of Man and Caernarvonshire, occurs abundantly in the form of small pebbles on the shore at Killiney, co. Dublin, doubtless derived from the "glacial gravels" of the coast. I have also found a pebble in the raised beach at Greenore, co. Down.

Mr. Teall's description of the rock of Ailsa Craig (*Mineralogical Magazine*, vol. ix. p. 219) enabled the very characteristic pebbles collected by Mr. Kendall to be referred to that mass as a source, or to formerly existing bosses south of or adjacent to it. As far as I am aware, all the material is in the form of pebbles, often only an inch in diameter. This is hardly likely to be its original condition, if removed by ice from Ailsa Craig, and is only one of many points that indicate a redistribution of our so-called "glacial" beds by subsequent action of rivers or other waters.

Grenville A. J. Cole.

Royal College of Science for Ireland, Dublin,
March 12.

THE SACRED NILE.

That Egypt is the gift of the Nile is a remark we owe to the father of history, who referred not only to the fertilising influence of the stream, but to the fact that the presence of the Nile and its phenomena are the conditions upon which the habitability of Egypt altogether depends. That that part of Egyptian archæology and myth which chiefly interests astronomers is also the gift of the Nile is equally true.

The heliacal rising of Sirius and other stars at the time of the commencement of the inundations each year; all the myths which grew out of the various symbols of the stars so used, are so many evidences of the large share the river, with its various water levels at different times, had in the national life. It was, in fact, the true and unique basis of the national life.

In this the Nile had a compeer, or even compeers. What the Nile was to Egypt the Euphrates and Tigris were to a large region of Western Asia, where also we find the annual flood to have been in ancient times a source of fertility over an enormous area which is now

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